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A CASE OF LEVITATION IN NEPAL

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THE following is a full and detailed account of an extraordinary, and to me inexplicable, incident which I saw in Kathmandu on Friday, 8 August 1941, and on which I made notes at the time. This account is based on those contemporary notes.

Before I describe the actual incident, some preliminary remarks are necessary. I was told that some Nepalis have to do periodic *puja* or sacrifice to their *Bhagwan* or spirit at their home village. If they fail to do this at the right time, the *Bhagwan* comes, wherever they may be, and possesses them. They lose consciousness, dance and shout and behave madly while under control. I had a young orderly called Krishna, aged about eighteen, who had to do this *puja*, but when the time came, he did not go to his home for the ceremony. Instead—or in consequence—he was suddenly possessed at eight o'clock at night in our servants' quarters. I went out and heard him making weird noises and raving. At nine o'clock the seizure passed, and our other orderly, the elder of the two, said he must be taken to his home (eight miles away) at once, or the *Bhagwan* would keep on seizing him all night. My Muhammedan bearer, Azmat, went also to help him home, and when all three came back next day, Azmat had a quite incredible story to tell of what he had seen and what had happened. As I was not there personally, I shall not trouble my readers with an account of it, but I told Azmat that if ever again a *Bhagwan* visited our compound he was to let me know at once. I should mention that Azmat, being a Muhammedan, had originally no knowledge or belief in Hindu *Bhagwans*.

A month or two later we had a little dinner party, and after dinner about nine p.m. we were sitting in the dark in the drawing-room admiring a film of big game shooting in Nepal kindly sent by H.H. the Maharaja, when Azmat came quietly into the room and whispered in my ear, '*Bhagwan phir a gaya*,' i.e. '*Bhagwan* has come again'. I did not want to disturb the party, so I slipped out

quietly, and went to the back of the house where the servants' quarters were. These quarters were a building of brick and tiled roof divided into five rooms in a row, each room about ten feet by seven and eight feet high, with a small extension at the back for cooking food, and a door three and a half by six feet high in front, and well lit with an unshaded electric light bulb. In one of these little bare rooms—there was nothing in the room except a roll of bedding in one corner and a small box in another—my elder orderly, aged about twenty-two and also called Krishna, was squatting quite alone on the bare floor, dressed in a shirt and khaki shorts with bare legs. His attitude was approximately as shown in the accompanying illustration (Figure I),¹ cross-legged with his hands clasped between his legs. His head and body were shaking and quivering, his face appeared wet with sweat, and he was making the most extraordinary noises. He seemed to me obviously unconscious of what he was doing or that a circle of rather frightened servants—and myself—were looking at him through the open door at about eight or ten feet distance.

This went on for about ten minutes or a quarter of an hour, when suddenly (with his legs still crossed and his hands clasped, see Figure II) he rose about two feet in the air, and after about a

*Figure I**Figure II*

¹ The illustrations were drawn to Mr Smythies's specifications by Mrs Elizabeth Sullivan in December 1950.—Ed.

second bumped down hard on the floor. This happened again twice, exactly the same except that his hands and legs became separated.

One of the servants whispered that the *Bhagwan* was very angry with Krishna and was punishing him by bumping him on the floor in this way, which, I must admit, was just what it looked like. The servants were becoming very frightened and worried, and I was feeling very creepy myself at this inexplicable sight. Then one of the Nepali servants produced a splinter of resinous wood, which he lit and placed the burning end in Krishna's mouth for a moment. The seizure continued unaltered for a brief interval, and then suddenly it passed, and Krishna opened his eyes and relaxed. He sat looking dazed but otherwise normal. Shortly afterwards I left and returned to our party, from which I had been absent for less than half an hour.

That is a detailed description of what I saw, and noted down the next morning. I cannot explain it.

To touch on one or two more points :

(1) I am quite convinced there was no fake ; the whole thing was unpremeditated and unexpected, with the orderly an unwilling, or rather unconscious, medium of an extraordinary manifestation.

(2) There was no possibility of any apparatus, such as a thin wire, having been rigged up to haul the man up and down. There was an excellent light, and I was quite close, and could see the whole of the small room. Nor could he have been pushed up from below, since he was sitting on a bare solid brick floor. When the levitation took place, the seven or eight other spectators, who were all my servants, were outside the room with me, and could not have ' assisted ' in any way.

(3) I was not in the least under the influence of alcohol (my limit is one ' chota peg '), and nor were the servants around me, who saw what I saw.

(4) It is, I think, an impossibility for anyone to *jump* from the position shown in Figure I to that in Figure II. At any rate I have tried it myself and cannot move an inch from the ground.

The Nepalis say that a *Bhagwan* regards the person he takes charge of as his own property, and never leaves him until death. Usually he exerts a beneficial influence, but at times when angry or annoyed, he takes violent control as exemplified above.

EDITOR'S COMMENTS

The incident described above was first brought to my notice by Dr J. R. Smythies, a member of the Society. I am greatly

indebted to his father, Mr E. A. Smythies, for agreeing to write an account of it.

The word 'levitation' is usually applied to a phenomenon in which the subject (or object) is alleged to remain suspended in the air for an appreciable length of time. In this sense, the incident described by Mr Smythies should not, strictly speaking, be termed a levitation. It should, therefore, be understood that in the discussion which follows the word is used for want of a more precise and convenient term.

Cases of alleged levitation among non-western peoples are usually reported by hearsay, less usually at second-hand. When there are first-hand accounts, which is rarely, more often than not they concern set performances given by a 'magician' or holy man, often with a subject or 'pupil', and are witnessed at a distance laid down by the demonstrator. The only two cases which I have been able to find in the Society's literature (*Journal*, 29, 275-7) exhibit some or all of these features. In both of them the word 'levitation' is used in its accepted sense, the duration of levitation in one case being given by Lord Halifax as 'half-a-minute or so'.

The account written by Mr Smythies is of interest for several reasons: (1) it is at first-hand; (2) it derives from notes¹ written at the time of the occurrence; (3) the incident was apparently spontaneous—that is, it does not appear to have been 'staged'; (4) the writer was able to observe it at a very short distance and in good light; (5) the subject was entirely alone in the room.

Mr Smythies has supplied the following further information at my request: (1) the story told by Azmat (referred to in the second paragraph of the report) concerned the apparent possession of the younger Krishna—and, simultaneously, of an uncle of his—but did not involve levitation; (2) Mr Smythies was not prepared for levitation to take place—'I was certainly *not*', he writes, 'expecting anything like levitation'; (3) the height of about two feet to which the elder Krishna was levitated 'was a sort of average, his feet a few inches lower, his seat about three feet up. That is the impression I have'; (4) Krishna did not immediately drop to the

¹ Mr Smythies informs me, in reply to my inquiry, that these notes were on some sheets of paper. He continues, 'Two or three years later, when my wife was writing her book *Tiger Lady* [not yet published.—ED.], she incorporated my notes into one of her chapters—"Life in Kathmandu". I have hunted through her sheaves of papers, and cannot find now my original notes, but, in case of any use, I enclose three pages of her original typescript. This was typed out years ago in Kathmandu (and is not just a recent addition).' The details of the levitation given in the typescript correspond with those in Mr Smythies's account, but there is no mention of the number of servants present, the distance of the witnesses from the subject of the levitation, or the conditions of lighting.

ground after each levitation—‘ I had the impression of one or two seconds pause at the top—not more.’ ; (5) all the servants who witnessed the incident were aware that Mr Smythies understood their language ; (6) there were no windows in any of the servants’ quarters. To my inquiry whether any of the servants said in so many words that they saw Krishna raised off the ground, Mr Smythies replied as follows :

I remember just after the levitation turning to my bearer (Azmat) who was standing near me, and saying in the vernacular, ‘ What is happening to him (Krishna) ’, and he said (also in the vernacular), ‘ The Bhagwan is very angry with him and is bumping him on the floor.’ This clearly suggested that he saw what I saw, and I never thought further of asking him, or the other servants, what he or they had seen. In fact it never even occurred to me that they could not have seen what I saw, and I had no reason (then) for trying to get corroborative evidence or further witness.

Mr Smythies does not know whether the elder Krishna or the other servants were prepared for levitation to occur. He has no information on the occurrence, or alleged occurrence, of the phenomenon in Nepal, but informs me that the Director General of Forests in Nepal at that time, Sir Kaiser Shumshere, to whom, a few days later, he described what he had seen, said that he himself had not seen or heard of such cases before, though he knew of *Bhagwans*. Mr Smythies is unable to say whether the subject of the levitation was in the ordinary course of events liable to epileptiform seizures, or whether the occurrence of epilepsy is marked in Nepal.

By good fortune, and through the kindness of Mr P. V. Killick, who is at present living in Kathmandu, and whose help I gratefully acknowledge, it has been possible to trace Mr Smythies’s former bearer, Azmat. (In writing to Mr Killick I did not, of course, say what the incident was, explaining that it was ‘ extremely important that if Azmat or any of his fellow-servants were now questioned about the happening, nothing should be said which would suggest a particular answer ’. It should, perhaps, also be said that Mr Killick and Mr Smythies are not known to each other, and that my letter was sent to Mr Killick without Mr Smythies’s knowledge.) It will be best first to reproduce the relevant passages in Mr Killick’s letter from Kathmandu (dated 8 February 1951).

I have examined Azmat and enclose a statement of his account of the incident as he related it to me through the interpreter.

The interesting thing, to me, is that, although he recollected the evening in question quite clearly, he insisted at first that nothing unusual had occurred. I then asked him if something had not occurred in which Krishna was the central figure whereupon he said ‘ Oh that ’

with an expression that implied his surprise and some amusement that anyone should be interested to hear about such an ordinary occurrence all these years later!

He and the interpreter say they have both witnessed many such incidents which they evidently accept as perfectly normal.

There are many stories of strange happenings among these simple people and even among Gurkhas in British regiments—as, for example, the British officer who went to a Gurkha soldier's hut and was saluted smartly by him outside only to enter immediately and see him on his bed undressed in an attitude of meditation.

In a postscript, Mr Killick writes: 'Azmat's account was confirmed by my old ayah who was also a witness.'

The report enclosed with Mr Killick's letter is as follows:

REPORT OF INTERVIEW WITH AZMAT

In August 1941, Azmat, the Indian bearer of Mr Smythies (then British Forestry Adviser to the Government of Nepal) went to the quarter of Krishna, Mr Smythies's orderly, and found him sitting cross-legged with arms folded and shivering with bloodshot eyes. Krishna said he was not feeling well. Azmat called Mr Smythies and some other servants who lit a Dhup. A Dhup is a paper container about the size of a cigarette containing incense and is lit and swallowed by someone supposed to be possessed by an evil spirit.

Krishna swallowed the Dhup and suddenly rose about three feet in the air remaining in the same posture with closed eyes and groaning. He rose and descended rapidly about three or four times in quick succession shaking violently.

Mr Smythies's gardener, who was present, is a Jharphuke (a man who specialises in locating and driving away evil spirits) and he then uttered in Nepali a Mantra (a charm) and placed a Tika (a small red circle usually of rice or paper symbolising rice) upon Krishna's forehead promising the evil spirit that Krishna would perform a Pooja (an offering to the gods) the next day. Krishna then slept.

The following day Krishna sacrificed a sheep at a temple and drank of its blood and the evil spirit was thereby placated and driven out of his body.

The reason for his being possessed by the evil spirit was said to be that he had failed to offer the customary annual sacrifice to the gods who were therefore displeased with him.

Both Azmat and the interpreter (an educated Newar) said that there was nothing unusual about this incident. People were frequently possessed by evil spirits when they had done something displeasing to the gods. Sometimes they rose up and down like Krishna. Sometimes they talked a lot of rubbish. Sometimes they recalled past events in which they themselves had never taken part or heard about. A Jharphuke is an ordinary working man who, by study and practice under a teacher, acquires powers of divination and expulsion of spirits. They are commonly used, for example, to name thieves or to detect and expel

evil spirits from a new house and apparently are not motivated by consideration of monetary gain.

Before writing to Mr Killick—which seemed to me to be a very long shot—I had obtained statements from Mrs Smythies and from Lt Col J. D. Ogilvy,¹ First Secretary in the British Legation (as it then was) in Kathmandu at the time, who were present at the showing of the film on the evening of 8 August 1941. Although these statements became less important as evidence after the arrival of the report of the interview with Azmat, they are of interest for the information which they give about Mr Smythies's reactions to his experience.

STATEMENT BY MRS SMYTHIES

5 January 1951

I remember very well how my husband described at our party on 8th August 1941 (the date is recorded in my diary) how he had seen our orderly possessed by a 'Bhagwan', and lifted up into the air by some impossible means. He was quite excited about it, but although he was perfectly sober, I found his account difficult to believe.

(Sgd) OLIVE SMYTHIES

STATEMENT BY LT COL J. D. OGILVY

12 January 1951

Your letter of 9th January [1951] re phenomenon reported by Mr E. A. Smythies on the evening of 8th August 1941 in Khatmandhu.

I am afraid that my memory is rather hazy about the event. I do however remember being present at a film show of big game shooting in Mr Smythies's house in Khatmandhu but do not remember any dates. Mr Smythies was called out of the room and when he came back stated that he had been summoned to deal with his orderly, who was behaving queerly in his quarters. He went on to state that the other servants said that a spirit had entered into the orderly and that he, Mr Smythies, saw the orderly levitated above the ground without any apparent effort or support. After a time the spirit was supposed to have left the orderly who came down to earth again.

I saw the orderly at various times afterwards but did not know him well—he struck me as being rather anaemic.

I did *not* see the phenomenon of levitation nor do I remember any further particulars.

(Sgd) JOHN D. OGILVY

¹ It may be regarded as relevant that Mr Smythies had lost touch with Colonel Ogilvy, and that I had to trace his present whereabouts myself. I need hardly say that no mention of levitation was made in my letter to him.

Dr E. J. Dingwall, to whom I am much indebted for the interest which he has taken in this case, has drawn my attention to a passage in E. T. Dalton's *Descriptive Ethnology of Bengal* (Calcutta, 1872). In discussing ceremonies occurring at the worship of Gansám¹ among the Muási (or Kúrs or Kúrkus), one of the tribes of the Central Provinces of India, Dalton quotes an account which he obtained from Captain W. S. Samuells, then Assistant Commissioner, Chútla Nágpúr. It was apparently in writing or by word of mouth, for Samuells does not seem to have committed it to print. The account, in which I have italicized those passages which show a particularly close resemblance to the descriptions given by Mr Smythies and Azmat, is as follows:

The Baiga [priest] is always the medium of communication, but he assembles the people to aid him in the invocation. Musical instruments are produced, dancing commences, and the invocation to the spirit is chanted until one or more of the performers manifest possession by wild rolling of the eyes and *involuntary spasmodic action of the muscles*. The affection appears contagious, and old women and others who have not been dancing become influenced by it in a manner that is horrible to contemplate. Captain Samuells, who frequently witnessed the incantation, is confident that no deception whatever is practised. As at revivals where similar scenes are produced by professing Christians, each person seized or exalted loses for a time all self-control, and the body, limbs, and neck are worked in the most exhaustive manner, till the Baiga interposes and relieves the victim.

The affection, says Captain Samuells, comes on *like a fit of ague*, lasting sometimes for *a quarter of an hour*, the patient or possessed person *writhing and trembling with intense violence*, especially at the commencement of the paroxysm. Then he is seen to spring from the ground into the air, and *a succession of leaps* follow, all executed as though he were shot at [possibly a printer's error for 'up'.—ED., S.P.R. *Journal*] by unseen agency. During this stage of the seizure he is supposed to be quite unconscious, and rolls into the fire, if there be one, or under the feet of the dancers without sustaining injury from the heat or the pressure. This lasts for a few minutes only, and is followed by the spasmodic stage. With hands and knees on the ground and hair loosened, the body is convulsed, and the head shakes violently, whilst from the mouth issues a hissing or gurgling noise. The patient next evincing an inclination to stand on his legs, the bystanders assist him and place a stick in his hand, with the aid of which he hops about, the spasmodic action of the body still continuing and the head performing by jerks a violently fatiguing circular movement. This may go on for hours, though Captain Samuells says that no one in his senses could

¹ The Gansám of the Muási deserves a footnote. He was said to have been formerly a chief who was devoured by a tiger just after his marriage. 'Cut off at such a moment', says Dalton, 'it was unreasonable to suppose that his spirit would rest.' He owed his apotheosis to the feat of having, one year after his death, visited his wife and got her with child.

continue such exertion for many minutes. When the Baiga is appealed to, to cast out the spirit, he must first ascertain whether it is Gansám himself or one of his familiars that has possessed the victim. If it be the great Gansám, the Baiga implores him to desist, meanwhile gently anointing the victim with butter; and if the treatment is successful, the patient gradually and naturally subsides into a state of repose from which he rises into consciousness, and restored to his normal state, feels no fatigue or other ill-effects from the attack. (pp. 232-3).

This is the only eye-witness account of phenomena at all similar to those described by Mr Smythies which I have been able to trace, even with Dr Dingwall's help.

In discussing the evidence in this case, the first points to note are (1) that the possibility of trickery seems remote, (2) that since Mr Smythies was not expecting levitation to occur, the hypothesis of suggestion is unlikely to cover his experience, even if one were to discount Azmat's evidence, (3) that Azmat's testimony corresponds with Mr Smythies's account in all material points as regards the levitation itself (which suggests that the incident must have made a strong impression on his mind at the time).

Mr Smythies says of Krishna that 'suddenly (with his legs still crossed and his hands clasped, see Figure II) he rose about two feet in the air'; Azmat's statement says that he was 'sitting cross-legged with arms folded', and that he 'suddenly rose about three feet in the air remaining in the same posture'. As both observers use the word 'rose', and as neither mentions any leap or spring, it appears that they both received the impression that no effort was involved. If Krishna did indeed rise two feet (or even one foot) clear of the ground by no effort of his own, then he did so by no known means.

It is, however, necessary to consider the possibility that, while appearing to the observers to rise without effort, he did in fact leap upwards. It is mechanically impossible for a person sitting with legs crossed and with hands in lap or arms folded to spring more than an inch or two clear of the ground, and this can only be done by swinging the body forward and then sharply backward in an unmistakable effort. (If the feet are tucked under the thighs, the body can be thrust just clear of the ground; if they are crossed in front of the seat, it cannot be done.) An experienced teacher of ballet very kindly made the attempt in my presence, with the results described. A European practitioner of Yoga was no more successful. (It must be recorded that she was confident of being able to propel herself at least two feet up into the air after a month or two of practice. As, however, she made several other claims—including the ability to make herself completely invisible

—but was not prepared to give a practical demonstration of any of them, this confidence was not impressive.)

Is it possible that Krishna, at the moment of rising into the air, was not sitting cross-legged but that he sprang up from a squatting position and at the same time drew his legs up under him so that he *appeared* to have risen with legs crossed? In this case he would have had to land in a squatting position after the first and second jumps (assuming that there were three in all) while after the third he would presumably have landed with legs crossed. It has been suggested that a very muscular person might be able to perform this feat with practice, but I have not succeeded in finding anyone willing to make the attempt.¹ It would certainly be difficult to do without having the arms free to maintain the balance. We cannot tell whether Krishna could have done it. He does not appear to have been of poor physique, for although he is described by Colonel Ogilvy as 'rather anaemic', Mr Smythies, who presumably knew him better, tells me that he was well built, muscular, and healthy, and puts his height at about 5 feet 8 inches and his weight at about 10½ stone. It seems unlikely that Mr Smythies, after having watched Krishna sitting on the ground with legs crossed for ten minutes, could have failed to notice if he had worked himself into a squatting position. Is it possible that he did notice it at the time, but afterwards overlooked it? Against this one must set the testimony of Mrs Smythies and Colonel Ogilvy. And would Mr Smythies have described himself as 'feeling very creepy at this inexplicable sight' if he had merely witnessed three leaps, however phenomenal?

Many of the details given by Captain Samuells are almost identical with those in Mr Smythies's description, and it is interesting to note that the duration of a quarter of an hour which he gives for the preliminary 'fit of ague' corresponds so closely with the 'ten minutes or a quarter of an hour' mentioned by Mr Smythies as the length of time that Krishna's head and body were shaking and quivering before he rose from the ground. Mr Smythies's comments on Captain Samuells's account (of which he had not previously heard) are as follows :

The 'wild rolling of the eyes and involuntary spasmodic action of the muscles' is very typical of what I saw in Kathmandu in Aug. 1941. Also the 'writhing and trembling with intense violence', the 'spring from the ground into the air . . . *executed as though he were shot up by unseen agency*', is a very good description, but it was hardly a spring.

¹ I should be interested to hear from anyone—ballet dancer, acrobat, or Yogi—who felt confident of being able to demonstrate this. A member of the Council of the Society has kindly offered to make a ciné film of any demonstration which seems likely to throw any light on the question.

It is a great pity that Captain Samuells, despite his insatiable curiosity,¹ did not describe the position of the possessed person at the moment of rising from the ground. Whatever it was, it is quite clear that he regarded him as propelling himself upwards by his own efforts.

Although it is a work which is based largely on hearsay, and should be read with great caution, Mme Alexandra David-Neel's *Mystiques et Magiciens du Thibet* (Paris, Plon, 1929) contains a passage which it may be interesting to quote. It describes the training of runners who are said to travel at great speed over long distances in Tibet in their task of summoning demons to the ceremony of propitiating Shinjed, the god of death. The passage is as follows :

The training consists of breathing and gymnastic exercises practised in complete darkness in a *tshams khang* in strict seclusion for three years, three months, three weeks, and three days.

Among these exercises there is one which enjoys the favour of many self-styled mystics who are not of an especially intellectual type. Not only members of religious orders but laymen also, both men and women, undertake prolonged 'retreats' in order to devote themselves to it. It is as follows :

The student sits cross-legged on a large and thick cushion. He inhales slowly and for a long time [*lentement et longuement*], as if he wanted to inflate himself with air. Then, holding his breath, and without using his hands, he jumps up, still keeping his legs crossed, and falls back without changing his position. Some lamas succeed in jumping to a very great height in that way [*sauter, ainsi, à une très grande hauteur*].

According to Tibetans, those who apply themselves to this method of training for several years become capable of sitting on an ear of corn without bending the stalk, or of standing on the top of a heap of barley without displacing a single grain. In fact, the aim is levitation.²

No indication of the height of these jumps is given. Nor are we very much wiser after reading, a few paragraphs later, Mme David-Neel's comment on a claim that after such training it is possible for a person to jump vertically with legs crossed to twice his own height. 'I have heard Tibetans from Kham maintain', she writes, 'that they have witnessed feats of this kind in their country.

¹ At the end of his chapter on the Muási, Dalton drily remarks that 'Captain Samuells was unable to induce a Muási to die, in order that he might, as an eye-witness, describe what then takes place'. (op. cit. p. 234). It may, of course, have been Dalton who omitted to quote Samuells fully as to the posture of the possessed person.

² I have translated this from the French edition (pp. 208-9) as the English edition (published in 1931 under the title *With Mystics and Magicians in Tibet*) is not only not an exact rendering but contains remarks which do not appear in the original.

However, those whom I have myself seen jumping have not seemed to me at all capable of such an exploit.' We are given no details of the personal experiences to which she refers.

To judge from the reported remarks of Azmat and the Newar, it would appear that the simple folk of Nepal regard levitation as one of the possible consequences of failure to do *puja*. If, as Mr Smythies's account leads us to suppose, the phenomenon does indeed occur, it is perhaps not beyond the bounds of possibility that its operation may be related to the unquestioning belief of the victim in its reality.

Mr Smythies appears to have witnessed something which Europeans are rarely privileged to see. One can only hope that the publication of this report will lead to the collection of further evidence which may throw some light on this puzzling matter.

E. O.

VISUAL HALLUCINATION OF THE SELF

BY J. C. FLUGEL

IN a recent number of the *British Medical Journal*¹ the distinguished French neuro-psychiatrist Jean Lhermitte, who has been on a visit to this country, contributes an interesting article on 'autoscopy' or 'visual hallucination of the self', in which he passes in review some of the numerous literary treatments of this theme and shows that clinical evidence provides many close parallels to the descriptions by the gifted authors who have been fascinated by this subject. The record of real or supposedly real cases of seen 'doubles' begins with Aristotle and continues in modern times with reports from a number of psychiatrists of the highest reputation. Goethe is among those who claim to have seen such a 'double' of themselves (though 'when I shook my head at the sight of this delusion it disappeared'). For the most part, however, it seems to appear to those who are suffering from some form of nervous or mental disease, such as epilepsy, general paralysis, encephalitis, post-traumatic disorders, or focal lesions of the brain—and to these mostly, though not invariably, when they are lost in thought or in states of drowsiness; while in the case of epileptics the vision seems liable to occur about the time of their seizures. With regard to these latter, there is a tantalizingly brief statement to the effect that the double has been seen not only by the patient himself but 'by others present during the patient's fit'.

¹ No. 4704, 3 March 1951.

If this is substantiated, we have presumably to deal with cases of collective hallucination ; otherwise we are faced only by a particularly interesting and impressive form of individual hallucinatory delusion.

This, however, raises problems enough, of great interest alike to the psychiatrist, the psychologist, and the student of psychical research. Attempted explanations of the phenomenon have been along two main lines, neurological and psychological. It is the former which Professor Lhermitte adumbrates in this article, his suggestions being based chiefly on the work of Henry Head and Gordon Holmes on the one hand and on that of Paul Schilder on the other. The first-named authors drew attention to the sensorimotor elements in the nervous system through which we become aware of our postural attitudes and the relative position of the various parts of our body, while Schilder elaborated these and similar findings into a more general treatment of the 'body-scheme' or 'body-image'. This image can be distorted through various diseases of the nervous system, though the precise nature of the lesions or disturbances involved is still to some extent hypothetical. Other interesting modern accounts will be found in articles by G. Bychowski ('Disorders of the Body Image', *J. Nerv. Ment. Dis.*, 1943, 97, 313) and W. Clifford M. Scott ('Some Embryological, Neurological, Psychiatric and Psycho-analytic Implications of the Body Scheme' (*Int. J. Psycho-Anal.*, 1948, 29, 141). The former, after describing various cases, quotes another authority (Menninger-Lerchenthal) as regarding 'autoscopia as a visual kinesthetic hallucination of the body-image, which becomes split by the action of a mechanism linked with the vestibular organ, the brain stem, and the parieto-occipital cortex'. From further considerations it would appear that the vision may be related to the failure of a 'binding process' connected with the parietal and thalamic regions, in the absence or disturbance of which process stimuli usually integrated with the body-image find a sensory centrifugal discharge and are attributed to the outer world.

The psychological explanation (to which Lhermitte does not here refer, but which of course does not exclude the existence of a neurological basis or predisposition) involves the psycho-analytic mechanism of 'projection', in which 'repressed mental processes, not recognized as being of personal origin, are attributed to the external world and (often) experienced as an outer perception'. The division into 'inner' and 'outer' under these circumstances frequently takes place along the lines of a crude separation between 'good' and 'bad' qualities. Most often it is the 'bad' aspects which are projected, as in paranoia, where the patient's own

unrecognized aggressive or sexual impulses may be attributed to others. This would fit in well with a case described by Lhermitte himself, in which the 'astral body' of a girl (who is described as 'good and pious') was felt by her to be 'in the grip of Satan', who would torture it and inflict upon it sexual outrages. (This patient had, it may be noted, suffered from epidemic encephalitis a few years before these phenomena appeared). Sometimes, however, it may be the 'good' part, the super-ego, which is projected—a process which is represented in certain current advertisements in which a shadowy admonitory self appears, and which occurs in Poe's tale of *William Wilson*.

A more complete understanding of autoscopy would probably involve a knowledge of its relations to certain other phenomena with which it may well have some important elements in common. Within the sphere of psychical research are the materializations produced by certain mediums, in so far as genuine phenomena of this kind may occur (though these differ from autoscopy in that they are seldom, if ever, 'doubles' of an actual sitter). More definitely germane to autoscopy are the appearances (visual or kinesthetic) of parts of a patient's body (as distinct from an apparently complete personality)—as in the feeling of 'phantom limbs' after amputation, or of supernumerary members (a case of a kinesthetically present third arm was, I understand, recently reported by a well-known neurologist), or of the patient mentioned by Bychowski 'who discovered parts of her body in the environment'. These merge into the numerous cases of general lack of appreciation of the spatial relations between various parts of the body or of their relative size or shape, of general disturbances in the judgment of spatial relations ('metamorphopsia' when this is visual), or of seeing objects double or multiple ('polyopia'); while in the last resort we find ourselves involved in the whole problem of distinguishing the self from the outer world.

Apart from hallucinations, and in addition to paranoia to which we have already alluded, we have such varied examples of projection as are involved in the 'imaginary companions' of children, in the process of falling in love (in which some psychologists have recognized as a significant factor the projection of the lover's 'good' aspects or ego-ideal), and in religious and political enthusiasms (in which there is often a 'splitting' along the lines of 'good' and 'bad', the good elements being ascribed to a divine being or human leader, the bad to a devil or to political, racial or national 'enemies'). Processes in some respects the opposite of these (inasmuch as they imply a reduction rather than an extension of the self or body-image) are to be found in the various kinds of depersonalization or of loss of the sense of unity with certain parts

of the body (' my hand doesn't belong to me ') even when sensation in the parts concerned seems to be intact. Allied to these in some ways, though with the significant difference that they are demonstrable in ' normal ' individuals, are the interesting results obtained in such experiments as those described by Werner Wolff in his *Expression of Personality* (1943). Here the subjects were shown photographs of their own hands, profiles, mirror writing, etc., along with three examples of each kind obtained from others. They could not as a rule pick out their own photographs, but, when asked to comment on the whole series, almost invariably displayed greater interest and affect (manifested both in approval and in disapproval or distrust) in their own samples than in those of others. They seem therefore to have had something like unconscious or repressed recognition, in which feelings towards the self played an important part. Finally, there are the phenomena of ' dissociated personality ', in which, too, the cleavage often seems to take place along developmental or quasi-moral lines (a secondary personality being in quite a number of the recorded cases more childish or irresponsible than the primary or ' normal ' one).

A deeper study of the similarities and differences in the physiological and psychological processes underlying these various types of phenomena would hardly fail to cast much further light upon the strange and disturbing experience of autoscopy. It is not without good reason that so many authors have been intrigued by the theme of the *Doppelgänger*, for it raises neurological, psychiatric, psychological, and philosophical issues which take us deep into the very heart of the most intimate of all our problems : that of human personality.

REVIEWS

'The Theoretical Implications of Telepathy'. By Margaret Knight. Article in *SCIENCE NEWS* 18. Edited by J. L. Crammer. Harmondsworth, Penguin Books, 1950. 1s. 6d.

The account by Dingwall and Parsons of the Shackleton experiments in card-guessing and the picture-guessing experiments of Whately Carington which appeared in *Science News* 9 has now been followed up, in *Science News* 18, by an interesting article by Margaret Knight on the theoretical implications of telepathy. The author, who was at one time on the staff of the National Institute of Industrial Psychology, is the wife of the Professor of Psychology in the University of Aberdeen.

Mrs Knight admits that recent experiments in ESP, such as the Shackleton series, appear to be impervious to criticism and she dismisses the usual objections such as faint sensory ones, faulty statistics, or unwarranted selection of data which have so often been urged by psychologists against the positive results obtained in this field. The most disturbing thing she says about the Shackleton results is that they involve precognition with its apparent implications that causation can work backwards in time. This precognitive aspect of ESP has been thought by many psychical researchers to preclude the possibility of our ever finding any *physical* explanation of telepathy. But Mrs Knight, like Professor J. B. S. Haldane, is not convinced that this argument is a sound one. She thinks it no more incredible that future physical events should influence a present physical event than that future mental events should be able to influence a mental event in the present. The author admits, however, that there is no experimental evidence whatever that a human brain can pick up radiations emanating from another brain, and no reason to suppose that even were this proved to be possible it would result in the owners of the two brains having similar mental experiences.

Mrs Knight is possibly right in saying that in the present state of our knowledge there is little justification for the oft-repeated assertion that 'telepathy is independent of space or distance'. I agree that a vast amount of systematic experiment with the same agent and percipient at varying distances is required before we can conclude that telepathic reception is unaffected by distance. It is also true that a good many long-range tests have produced results which are only of borderline significance. I do not think, however, that the author is factually accurate in her statement that 'the longest distance over which results of undoubted significance have been obtained is some 200 miles—between London and Merksem in Belgium' (p. 14). Rhine himself has reported highly significant results over longer distances in his first book *Extrasensory Perception*.

While Mrs Knight thinks that no hypotheses put forward to account for telepathy and precognition are entirely plausible, she considers that the late H. F. Saltmarsh's theory of the extension of the 'specious present' is the best we have so far achieved with regard to precognition. This theory would naturally appeal to a psychologist, but it seems to me that the theory succeeded by evading the formidable metaphysical difficulties of the problems. It was, however, a brilliant effort. There is no mention of Carington's 'Association Theory' of telepathy, but the author appears to favour the view that at the subconscious level minds are not entirely separate entities. If it is true that electrical and chemical

changes in the brain of A can produce conscious experiences not only in the mind of A but also in the mind of B, then we are driven to the conclusion that the two minds are not entirely distinct. Since no psychologist has been able to explain how a physical event or events in A's brain can give rise to a conscious experience in A's mind, then psychologists who swallow this miracle without any fuss ought to be prepared to accept the further miracle that occasionally physical activity in A's brain may give rise to a conscious experience in B's mind. And why not, if mental experiences are not spatial events?

The author suggests that ESP does not mark a new stage in evolution but may be the vestige of a primitive faculty derived from the lower animals and which is best observed in the behaviour of birds and social insects. Whether we agree or not with the author's views, they are expressed with scrupulous fairness and it is surely significant that a professional psychologist should endorse the opinion of another distinguished psychologist, Dr Thouless, that ESP has now been established beyond reasonable doubt and that it is a waste of time to conduct experiments merely to demonstrate the existence of the faculty. Our efforts should now be directed towards an understanding of the conditions under which it occurs and the practical control of its functioning.

S. G. SOAL

SIXTY YEARS OF PSYCHICAL RESEARCH : Houdini and I among the Spiritualists. By Joseph F. Rinn. New York, Truth Seeker Company, 1950. xviii, 618 pp. \$5.00.

Mr Rinn is a business man who, according to the Introduction to his book, 'began his long public career [i.e. as investigator] in an earnest endeavour to discover scientific evidence of a future life. He realised that once communication was established with the dead and we beheld the spirits of the departed, all doubts would be dispelled concerning a life after death . . .' It was not unnatural that his endeavour to behold the spirits of the departed should have led him into the shady bypaths of the psychic underworld, or that there he should have met nothing but fraud. This, as was also natural, if regrettable, produced a violent reaction against all forms of mediumship, and a bias against all psychic phenomena, mediumistic or not.

To quote the Introduction once more : 'A member of the British Society for Psychical Research, Mr Rinn withdrew from the organisation when he painfully realised that it was more concerned in protecting and pampering mediums, and in covering their tracks, than in exposing them'. Mr Rinn's sole connection with the S.P.R. was that he was an associate of the American

Branch of it for about four years, from 1897, during the period when the Society strictly adhered to its policy of refusing to investigate mediums previously detected in deliberate trickery, a strange way of 'protecting and pampering' them! This was not a very long time in which to form a considered judgment on a subject as complex and obscure as we all know psychical research to be. But then for Mr Rinn there are no complexities, nothing more than fraudulent mediums, a gullible public, and gullible or dishonest investigators. What need for psychological subtleties? Is not Mr Rinn a trained conjurer, and cannot he at a pinch consult Houdini?

How magnificently that oracle disposed of hypnotism as 'a big fake'! Even Mr Rinn was inclined at first to jib at this sweeping pronouncement, but all his doubts were dispelled when Houdini introduced him to a man who claimed to have been a paid confederate of Charcot. Whether or not there is any truth in the story to which this self-accused confederate confessed, I do not know, but it would in any event have little bearing on the status of hypnotic research. Hypnotism, it is well to remember, was put on a scientific basis as the result of the long labours of many scientists in different countries. For a time the French took the lead, and among the various French schools of the Salpêtrière, Nancy, etc. there was vigorous mutual criticism. Any taking of results on a large scale by one school would quite certainly have been denounced by the others.

But Mr Rinn has a tenderness for the 'confessions' of self-confessed scoundrels implicating alleged confederates who are not in a position to reply, as is shown by his keenness to swallow the 'confession' of Blackburn, who, however, was careless enough not to ascertain that the man against whom he 'confessed' was not dead, as he supposed, but very much alive and kicking: see *S.P.R. Journal*, XV.

Mr Rinn is much too fond of charges of dishonesty against other investigators, which he scatters as freely as he does challenges of thousands of dollars to anyone who will produce genuine psychic phenomena, under conditions, of course, that he approves. The failure to take up these challenges he regards as proof that no genuine phenomena occur, but his own controversial manners have to be taken into account, as in his attack on our former President, Walter Prince (pp. 469-70). It appears from the *New York American* of 19 August 1924, which he quotes, that Prince and Houdini were debating the reality of psychic phenomena in a New York church when 'Joseph F. Rinn entered the discussion with a direct attack on Dr Prince's sincerity'. The meeting broke up in disorder and as the crowd pressed to the doors Mr Rinn

declared, 'That man is a liar because he makes his living out of that nonsense'.

Prince was at that time risking his livelihood by the firm line he was taking as a member of the Committee appointed by the *Scientific American* in criticising 'Margery'. Her supporters at that time dominated the American S.P.R., and Prince lost his position as Research Officer of that Society in consequence, as well as incurring much personal abuse. Houdini, Prince's colleague on the Committee, testifies warmly to his integrity, and so would many still living in America and this country who remember him with honour and affection. But without compunction or apology Mr Rinn twenty-six years later repeats the calumny.

Just what are Mr Rinn's qualifications to pose as a judge of psychical researchers? What sort of investigations has he conducted and with what subjects? The Introduction says, 'The pageant of seance deceptionists passes before the eye in colourful succession', and about two dozen names are given. If the list be analysed, it would appear that in his unregenerate days, while he still had an open mind, he had contacts with two of the Fox sisters and Slade; that later he had a single sitting with Mrs Piper, and another with Eusapia Palladino; and that the rest of the list consists either of mediums with whom he never sat or persons whose importance in psychical research was not equal to their local and transient notoriety. I have not noticed in the book any mention of investigations by Mr Rinn of poltergeists, haunted houses, apparitions, or other spontaneous phenomena, or any experiments by him in telepathy or clairvoyance, apart from the exposure of sundry public performers.

Let us consider whether in his two sittings with Mrs Piper and Eusapia respectively he did anything to advance knowledge of their mediumships. In 1896 Mr Rinn visited a 'temple' in Boston where a medium, Concannon, and his wife produced materializations which he recognized as fraudulent. Shortly after his visit some of his friends made a thorough exposure, grabbing a 'spirit' robe, wig, etc. and leaving a nearly naked Concannon. After quoting a newspaper account of the incident, Mr Rinn continues:

This exposure should have shaken my belief that a genuine medium existed, but the manifestations in Mr Ayer's temple [where the exposure had taken place] continued to be regarded by Spiritualists as undoubtedly genuine, and the members of the Society for Psychical Research did not lose faith. We had been fed with stories of the wonderful performances and psychic power of Mrs Leonora Piper, although a preliminary report had not yet been issued by Dr Richard Hodgson, who had her in charge.

This short paragraph is replete with absurdities. Why should the exposure of Concannon, whose fraud was of a very gross kind, cast doubt on mediumship in general? If the members of the S.P.R. did not 'lose faith' in Concannon, as Mr Rinn's sentence suggests, it was because the majority had never heard of him and had no faith in him to lose. Why bracket him with Mrs Piper, whose phenomena were of quite a different type? As for Dr Hodgson not having produced a preliminary report on her, two long reports on her had already been published in S.P.R. *Proceedings*, one (vol. VI, 436-659) by Myers, Lodge, Leaf, and James, and the other (vol. VIII 1-167) by Hodgson himself.

These reports had established several points concerning the Piper mediumship, such as (a) that it was difficult to take the Controls, e.g. Phinuit, unreservedly at their face value; (b) that sometimes they made incorrect statements and were unable to answer questions correctly; (c) that the medium, when in trance, could and probably did pick up information from the sitters by muscle-reading (see *Proceedings* VI; Lodge's remarks on p. 451, and Leaf's on p. 562),¹ but also (d) that there was no ground for supposing that in her normal state she obtained information as to the sitters, their friends etc. and (e) that, when all allowances had been made for (a), (b) and (c), she had genuine psychic powers. When, therefore, in an interview with Hodgson after his sitting in 1896, Mr Rinn brought up muscle-reading, etc., etc., as a sufficient explanation of Mrs Piper's phenomena, it was not unnatural that Hodgson, with his much longer experience of mediumistic trickery, should have shown impatience. Mr Rinn suggests that this was because Hodgson was 'of the English gentleman type' and unduly touchy. Hodgson was in fact a very unconventional, plain-spoken Australian, well accustomed to the rough and tumble of controversy.

At the time of Mr Rinn's sitting the principal Control and one of the principal Communicators at the Piper sittings was George Pellew, who had died in 1892 and is called in the printed records 'George Pelham' or 'G.P.'. In 1921 Mr Rinn learnt to his 'amazement' (p. 175) 'that documentary evidence existed from the family of "G.P." that a fictitious story was built about the life of his former friend by Dr Hodgson to justify his change to the spiritistic hypothesis, and that most of the statements made about "G.P." in Dr Hodgson's report [in S.P.R. *Proceedings*, XIII] were *absolute falsehoods*'. (The italics are Mr Rinn's). On p. 180 he says, 'Dr Hodgson lied outrageously on many important points in relation to "G.P." and his family.'

The only evidence Mr Rinn puts forward in support of these

¹ In the later stages of the mediumship this could not occur.

sweeping statements is a letter written in 1918, more than twelve years after Hodgson's death, by 'G.P.'s' brother, Professor Pellew, to Clodd, the Rationalist author. It is a long letter, taking up nearly four pages of print. The first half of the letter is to the effect that the Pellew family refused to accept the 'G.P.' communications as coming from the real George Pellew, mainly because they did not reflect his intellectual standards. This was a matter of opinion, and there is no suggestion of any false statement by Hodgson. The latter part of the letter does accuse him of having in one instance misrepresented the facts, not about 'G.P.', but about a sitter, John Fiske's, opinion of his sitting. Professor Pellew had been shown by his parents 'a curious letter from Hodgson. It was somewhat to this effect'. The substance of the letter, as so recollected, was that after a Piper sitting Fiske had told Hodgson that he was *absolutely convinced* that he had been talking to his old friend, George Pellew. A few weeks later Professor Pellew, as he says in his letter to Clodd, met Fiske, who stigmatised Mrs Piper as 'that old fraud', and denied that he had ever thought that through her he had conversed with George Pellew.

The Fiske story is, of course, the loosest hearsay, depending on Professor Pellew's admittedly vague recollection ('somewhat to this effect') of a letter he had seen at some unspecified previous date and of his memory of a conversation, also of uncertain date, with Fiske. It would not begin to be evidence without having the exact terms of Hodgson's letter, and a first-hand statement by Fiske as to exactly what his comments on the letter were. He should also state when the sitting in question occurred and what report on it, if any, he gave to Hodgson at the time. This might enable some contemporary written record of the sitting and of annotations by Fiske to be traced, and these would of course be evidence worth attention.

The story given in the letter is not a plausible one. By the testimony of all who worked with him in this country or America, whether or not they agreed with his views, Hodgson was a man of honour. But even if they had all been mistaken on this point, he could not have risked misrepresenting things that happened at or in connexion with sittings which he was supervising. If any of the Pellew family, or Fiske, or anyone else had during Hodgson's life reported a single case of misrepresentation by him to any member of the S.P.R. Council in England or to any of his American colleagues, and had been able to substantiate the charges, that would have been the end of Hodgson's career in psychical research.

'Mrs Piper's Confession. Disclaims Contact with the Spirit World.' So runs the heading of one of Mr Rinn's chapters

(p. 195). By this time it will come as no surprise to the reader to learn that Mrs Piper never made a 'Confession' at all. In the summer of 1901 Mrs Piper gave an interview to a journalist, and on 20 October of that year the *New York Herald* published a long statement purporting to be made by her, and in fact based on the interview. The statement contains the following passages: 'I have always maintained that these [psychic] phenomena could be explained in other ways than by the intervention of disembodied spirit forces. The theory of telepathy strongly appeals to me as the most plausible and genuine scientific solution of the problem.' And later on, Mrs Piper says, or at any rate, the journalist reports her as saying: 'I do not believe that spirits of the dead have spoken through me when I have been in a trance state.' On Mrs Piper's attention being drawn to the *New York Herald* article she promptly dictated a statement which appeared in the *Boston Advertiser* of 25 October 1901: 'I did not make any such statement as that published in the *New York Herald* to the effect that spirits of the departed do not control me. . . . Spirits of the departed may have controlled me and they may not. I confess that I do not know.' This latter statement Mr Rinn does not quote. But whether or not the *New York Herald* was justified in reporting Mrs Piper as saying she did not believe the spirits of the dead had spoken through her, the use of the word 'Confession' by Mr Rinn was quite unjustified. For many years prior to this interview two views of the Piper communications had been discussed by psychical researchers. One was that they came from spirits of the departed; and the other was that Mrs Piper's subconscious mind received the substance of them by telepathy, the Controls and Communicators who manifested in the trance being subconscious dramatisations somewhat akin to secondary personalities. The second hypothesis, which Mrs Piper according to the interview preferred, no more implies dishonesty, as the word 'Confession' does, than the first. It appears from the report on the episodes in S.P.R. *Journal* X, pp. 142-3, 150-2, that the *New York Herald* had by way of 'advertising smartness' made an advance announcement of her 'Confession', and that in response to a protest by her, the paper assured her that word would not appear in the actual article, as in fact it did not.

Eusapia was more up Mr Rinn's street than Mrs Piper, and one might have hoped for some new light on her from Mr Rinn's sitting. This took place on 17 April 1910, and is described, together with the preparations for it, on pp. 278-81 of the book. It had at this time been long known and was generally accepted that Eusapia would use whatever trickery the conditions of control permitted. The question was whether she could produce her

phenomena under conditions effectively excluding trickery. The group of highly competent investigators who sat with her at Naples in 1908 believed she could and did : see their report in S.P.R. *Proceedings* XXIII.

In 1909 she came to America and in 1910 gave a series of sittings to a group connected with Columbia University. A member of the group invited Mr Rinn and some friends of his, who were experts in trickery, to attend a sitting. Before this sitting Mr Rinn and his friends arranged an elaborate plan to trap the medium, some, but not all, of the members of the University group being privy to it. Part of the plan was that at a particular point in the sitting and for a prearranged time Mr Rinn's friends, who were acting as controllers of the medium, should deliberately release their control. This plan was put into action, and during the prearranged relaxation of control Eusapia produced phenomena, which she could not do during the part of the sitting when the control was strictly maintained. The result did no more than confirm what had already been established as to her mediumship fifteen years earlier at the Cambridge sittings. The S.P.R. has always maintained that it is possible to test a medium without laying traps, and that complete candour between fellow-investigators is imperative. Departures, such as Mr Rinn's, from the code of mutual confidence between investigators merely open the door to the bogus investigator, who is as much a hindrance to serious research as the bogus medium.

It would be tedious to correct all Mr Rinn's minor inaccuracies: here are a few jotted down as I read the book. Barrett was not 'head of the British S.P.R.' at the time of his American visit in 1885, or anywhere near it (p. 15). J. H. Hyslop is made (p. 294) to speak in 1910 of 'the English branch of our association', which he would certainly not have done, as he had negotiated on the American side the complete separation of the British and American Societies in 1906. Ivor Tuckett was not at any time a 'prominent member' of the S.P.R. (p. 309) : in 1911 he was not a member at all. It is correctly stated (p. 599) that in 1938 the ESP cards used by Professor Rhine were unsuited for experimental purposes : see S.P.R. *Journal* for May 1938. Professor Rhine was by that time quite aware of the defect, and was arranging for the use of a better type of card in his later experiments: Mr Rinn does not mention this. Nor does he, after mentioning (pp. 596-7) the negative results of Dr Soal's earlier experiments as tending to disprove Professor Rhine's claims, anywhere refer to the positive results that Dr Soal and Mrs Goldney later obtained through Shackleton (S.P.R. *Proceedings* XLVII). It is not true, as suggested on pp. 291-2 of Mr Rinn's book, that luminous paint is only used by

'crooked mediums'. Until the development of infra-red technique it was the standard method of indicating the situation of persons and objects at sittings held in poor light, and has often in that way been used in our seance-room. Mis-spellings of names familiar in psychical research are common; 'Phenuit', 'Mrs Sedgwick', 'G. B. Door', 'Rev. Charles Tweedle', 'Valentine'. On p. 444 the names of ten members of a Committee to investigate spirit photography are given: five are mis-spelt.

A large part of the book is taken up with one-sided accounts of conversations in which Mr Rinn scores off the other fellow. One's confidence in the accuracy of these accounts is shaken by Mr Rinn's exaggerated bias, and the prevalence of blunders large and small destroys it entirely, beyond hope of restoration by the most copious extracts from the American press, to which he freely resorts.

The pity is that Mr Rinn, whose knowledge and experience of mediumistic trickery would have qualified him to write an interesting book of value to psychical research, if he had confined himself to matters that he understood and that had come under his own observation, has been so unwise as to go outside these limits. Knowledge of methods of deception is needed in psychical research, and the S.P.R. has always been fortunate enough to include members well versed in them. But such knowledge does not by itself make a psychical researcher, without more accuracy, a better idea of evidence, and a greater understanding of human nature, whether in its normal or abnormal states, than Mr Rinn shows in this book. To succeed in business he must have had a fair share of these qualities, but when it comes to psychical research they fail him. No doubt this is because for him the subject has never been one of impartial enquiry. He began with a desire to obtain evidence of a future life, proceeded to take the worst kind of route to that objective, found that it led nowhere, and went violently into reverse for the rest of his life with no better results. Let the sad fate of Mr Rinn stir us all to examine our consciences to see whether we are ourselves free, to quote our Society's inaugural manifesto, 'from prejudice or prepossession of any kind'!

W. H. S.

PSYCHOTIC ART. By Francis Reitman, M.D., D.P.M. London Routledge & Kegan Paul, 1950. x, 180 pp. 17 plates. 16s.

Although the artistic products of the mentally unbalanced have long been known, little serious attention was given to them until the nineteenth century when such students as Marcé and Simon in France (1864 and 1876), Lombroso in Italy (1880), and Kiernan

in the United States (1892) began to view them, not only from the standpoint of the art critic, but also from that of the practising psychotherapist. In later years, following the more detailed work of Prinzhorn at the Heidelberg Psychiatric Clinic, came further work from men of such diverse views as Vinchon, Asschaffenburg, Pfeifer, and Schilder.

In the present work Dr Reitman, who has had the advantage of working with Dr E. C. Dax at Netherne House, has attempted to examine the problems inherent in psychotic and in particular schizophrenic art from the point of view of the neuro-psychiatrist, and to show what he calls the biologically determined dynamics of the material at his disposal. He regards the art products of the schizophrenic patient as a kind of active and creative ritual. Due partly to the distorted view of his own and other body-images (body-schemata) and partly to his disordered concept of space-relations, the artist paints in order to adjust himself to his own changed view of reality. His productions are thus in a sense pictorial representations of his own disturbed thinking, and can therefore be evaluated in psycho-physiological terms. It is, Dr Reitman insists, a mistake to appraise them solely from the psychological standpoint, since they contain both motor and sensory phenomena, and must therefore be approached from a biological as well as from a psychological angle. It is here that the author parts company from some of his predecessors, notably Prinzhorn, and from those who, from their psycho-analytic training, tend to find in every schizophrenic painting those signs and symbols which form the basis of much of their psychological interpretation in other fields.

From what Dr Reitman has written it is clear that he regards schizophrenic art as primarily dependent on the patient's own abnormal cognitive condition. From his changed mental position both as regards himself and the external world the schizophrenic is, in one sense at least, maintaining his own existence and adjusting himself to a new situation. Thus any analysis of such paintings must give due allowance to a psychological approach, whatever stress be laid on the purely neuro-psychiatric interpretation.

It is here that the psychical researcher may perhaps be pardoned if he ventures to express an interest in the total lack of any reference in Dr Reitman's book to mediumistic art. Apart altogether from such important and complex cases as that of Thompson-Gifford, it is, to say the least, rather strange that the author has not thought fit to mention the work of such well-known automatists as Mrs W. M. Wilkinson, Gilbert Cooke, F. L. Brown, A. Machner, F. Gentes, or Mrs I. T. Bush. Had he done so, and noted the many striking parallels between schizophrenic art and

that of the automatists, he might have made an effective addition to his discussion of the comparison between the paintings of schizophrenics and those of certain modern artists, while some similarity between these two classes of product is obvious. Dr Reitman is at pains to point out that this is due to the modernistic technique of analysing a system of relations which necessarily leads to fragmentation. On the other hand the same fragmentation is seen in psychotic art, but here it is not due to any system of conscious analysis but rather to the basic disintegration of the personalities of the artists.

From his treatment of psychotic art as seen psychologically it is clear that Dr Reitman has very little patience with the elaborate interpretations of certain of the followers of Freud and Jung, although at the same time he does not seem to distinguish very clearly between the teachings of master and pupil. In this connexion his short treatment of some of Goya's products appears rather unconvincing, and had he chosen Bosch instead of the Spanish artist the reader would have been much more interested in his diagnosis. His discussion is interesting, however, since he shows that, in his opinion, the content of the *Caprichos* can reveal material which is pathognomonic of schizophrenic experience, although how far he would go in a further interpretation remains uncertain.

To sum up, then, Dr Reitman has written a thought-provoking book which should be read by all who are interested in a little understood subject. Although the list of authorities is brief, it contains a representative selection of easily available material, while those who require further references, can turn to the papers of Anastasi and Foley, although these authors, like Dr Reitman, seem to ignore most of the material on mediumistic art.

E. J. DINGWALL

THE MYSTERY OF DREAMS. By William Oliver Stevens. London, Allen & Unwin, 1950. vi, 280 pp. 16s.

Mr Stevens has given us a book of a kind badly needed, one that should put before us a large number and great variety of dreams, should classify them according to their significance for psychical research, and should comment on the points that distinguish one example from another. The scheme of the book appears from the chapter headings. First come The Dream in Literature, Symbolism in Dreams, and The Solution of a Problem (i.e. dreams in which a problem is solved for the sleeper): then follow chapters in which Telepathic, Clairvoyant, Warning and Prophetic, Borderland, Concurrent and Reciprocal dreams are successively passed in review, with, to end, some cases difficult to classify.

In the concluding chapter the author explains the principles on which he has selected his examples. He mentions first readability, that is 'variety in time, place, characters, and especially in type of incident', but adds that primarily the selection was 'based on what seemed to be authenticity'. In all the cases cited he is convinced that the narrators believed what they said, although some of them date from an uncritical age, and for others corroboration could not be obtained. With this caution in mind, the reader can rejoice that the author has cast his net so wide. Cases reported in good faith but falling short of the highest evidential standards can be read 'as Hierome saith' of the Apocrypha 'for instruction—but not to establish any doctrine'. Many of the cases cited are drawn from the *Proceedings* and *Journal* of our own and the American S.P.R. Others are taken from books of various kinds, and many were reported direct to Mr Stevens by correspondents in the United Kingdom and America.

Mr Stevens is impatient (pp. 57, 58) of attempts to explain dreams by bringing in 'the old reliable pack horse, the subconscious mind'. Most people, he truly says, 'are quite hazy as to what the subconscious mind actually is and how it operates'. In the present stage of our knowledge it is well not to attempt too precise a definition of the subconscious mind. Many psychical researchers would not accept the author's description of it as 'a complete record of our lives': 'complete' is too wide, and 'record' suggests passivity. It looks as if he supposed that the champions of the subconscious regarded it as the ultimate source of all paranormal cognition, and not, as most of them do, as the potential vehicle, and in many cases the probable vehicle, for material of external origin.

W. H. S.

THE REVOLT AGAINST REASON. By Arnold Lunn. London, Eyre & Spottiswoode, 1950. xi, 252 pp. 15s.

The author of this lively and stimulating book writes with the enthusiasm of the Catholic convert, and displays an extensive acquaintance with the writings of philosophers and theologians from antiquity to the present day. One wonders, however, whether the abundance of often lengthy quotations is due to a naive desire to show that he is in good company in his opinions, i.e. an appeal to authority, or due to the urge 'to add verisimilitude. . .'. His tale, however, though in many places unconvincing to the non-Catholic, is never bald. Mr Lunn starts from the premise that the Catholic religion is an essentially rational system based on Revelation and the Aquinate 'proofs' for the existence of God, and that faith derived from mystic experience is almost an

unnecessary and not always welcome adjunct. Luther, he maintains, by exalting mysticism and denying the relevance of reason in matters of religion, was the initiator of the 'Flight from Reason' and of the subsequent—according to Mr Lunn, consequent—decline in morality, ethics, and aesthetics which has reached its climax in our day, especially in the dictatorial systems. This theme is developed with much ingenuity.

A large part of the book is taken up with a 'debunking' of Darwinism and a vigorous attack on the 'scientians', a term coined by Mr Lunn to describe those materialists and cryptomaterialists who deny the supernatural and affirm the omniscience of natural science. In the Western world at least, this type is fast becoming extinct and thus the author's attacks, which are mostly well-reasoned, apply largely to Victorian and early twentieth century scientists. He makes much of the practice among scientists of often ignoring facts which are inexplicable by, or in contradiction to, a current theory. In this context his examples are drawn mainly from biology and the realm of paranormal phenomena. If Mr Lunn had been an experimental scientist he would have known that in order to construct any theory it is not only permissible but essential to neglect some facts; otherwise—human limitations being what they are—it would be impossible to formulate any theories at all. The successive theories in any subject embrace an ever-increasing number of hitherto ignored facts. His argument, that because the theory of evolution cannot account for certain facts, therefore special creation must be true, shows that he does not comprehend the true role of scientific theory. No theory can be completely adequate to its subject, and a theory has fulfilled its purpose when it is seen to be inadequate and is superseded by a new one. Furthermore, he fails to appreciate that a theory of special creation is heuristically useless.

As far as paranormal phenomena are concerned, the pretext for ignoring them among many scientists was, and still often is, disbelief. The true reason, however, is that they seem irrelevant and cannot be fitted anywhere into the present world picture. It is just as unreasonable to reproach Victorian and later scientists for ignoring paranormal phenomena as it would be to reproach Galileo for ignoring magnetism and electricity. The phenomena available at the time just did not fit in anywhere into the current view of the universe. That is why the present state of psychical research is as good an example as any in the history of science, of the necessity for a working hypothesis before systematic experimental work can be planned.

To the non-Catholic many arguments in this book are difficult to swallow. In particular, in discussing the dichotomy between

materialists and theists, Mr Lunn seems to be unaware that many intellectually honest people are content to be agnostics and to base their morality on intuitive feelings of human dignity.

This is definitely a thought-provoking book.

P. H. PLESCH

JOURNAL OF PARAPSYCHOLOGY, Vol. 14, no. 4, December 1950.

Durham, N.C., Duke University Press. \$1.25.

Professor Rhine makes a mid-century appraisal of the present position in parapsychology. He lists as solved problems the separate occurrence of ESP and PK, the occurrence of clairvoyant ESP, of precognition, and, as a deduction from these, the non-physical nature of psi. He indicates more problems not yet solved or not fully solved, of which examples are: the experimental demonstration of telepathic ESP, the possibility of conscious control of psi, and the biological nature of psi capacities.

Remi Cadoret and Dr Pratt report a tendency to consistent missing in ESP experiments, that is for the guessing of one face when another was really the target. With the help of Dr Greville they have evolved a method of dealing with the somewhat difficult statistical problem of assessing the presence of such a tendency.

W. A. McElroy and Miss Winifred Brown report an experiment done at Glasgow University on the effect of giving mild electric shocks as punishments for incorrect responses in an ESP experiment. It was hoped that this method would make learning possible. This hope was not fulfilled, since in the series in which shocks were given there was a significant decline effect. The rate of scoring in the series of experiments with shocks showed an over-all significant deviation from mean chance expectation, while a series without shocks showed a much smaller positive deviation.

Olivia Rivers gives a brief account of an experiment designed to test the correlation between psi capacity and the results of a mental health rating. Although her subjects showed significant scores in an ESP test, these were found to be not significantly correlated with their mental health ratings.

An exploratory experiment by J. B. Rhine on two water diviners showed interesting results of which the principal was a strongly significant tendency to score below mean chance expectation. Professor Rhine is inclined to attribute this result to the mental process connected with the experimental situation.

R. H. THOULESS

CORRESPONDENCE

METAPHYSICS AND PARAPSYCHOLOGY

SIR,—Dr Rhine's Myers Lecture *Telepathy and Human Personality* contains a plea that in pursuing parapsychological research we shall free ourselves from 'speculative associations and cultural trappings' (pp. 30-1) and from 'traditions of theological and occult character'. I suggest that it is impossible completely to avoid making metaphysical assumptions in our scientific thinking, and that Dr Rhine's lecture is an illustration of the truth of this.

To begin with, he assumes, as most psychical researchers do, that we can distinguish telepathy from clairvoyance. I do not quarrel with this distinction if it is merely one of scientific method, but I must insist that if we regard it as more than this, we have adopted a metaphysical theory. For there is *one* type of metaphysical theory, that of modern objective idealism, for which this distinction vanishes. On this view, 'object' and 'subject' are abstractions, the concrete ontological unit containing both subject and object, and therefore what we call clairvoyance *is*, in a broad sense, telepathy, for the object perceived in clairvoyance is always known to a superconsciousness, or oversoul, or to the orthodox Christian God who can be regarded both as transcendent and as immanent in finite beings.

Both in Dr Rhine's lecture and in the Thouless-Wiesner four possible interpretations of 'person to person transference of thought impressions' (p. 19) we see the influence of a realist metaphysic which is just as 'speculative' as the idealist. Indeed, Dr Rhine's statement on p. 29 of 'the primary question of psi research' almost suggests that he has not completely freed himself from the influence of the crude materialism which held that the brain secretes thought as the liver secretes bile. He states that the primary task is the discovery 'whether there is an immaterial part of human personality, a spiritual self that might conceivably be considered capable of survival'. Here we see clearly the metaphysical assumption that whatever else may *not* be real, the body as a 'thing-in-itself' indubitably *is*. Now quite apart from the idealist metaphysic, which has been held by thinkers of the calibre of Hegel, Edward Caird, T. H. Green, F. H. Bradley, Bernard Bosanquet, W. T. Stace, and Brand Blanshard, what about the physicists' discovery that the notion of a three-dimensional 'block Universe' in a common time order must be given up and that we must reason *as if* the 'thing-in-itself' were a four-dimensional

continuum which must be measured by 'complex numbers'? What about the discovery that even the electron cannot consistently maintain its objective substantiality but becomes a wave—and a wave of what? It is matter, not the spiritual self, that is doubtful today. For although the four-dimensional continuum, tensors, matrices, and all the rest of the mathematical physicist's stock-in-trade cannot safely be regarded as more than methodological devices to enable observers to adapt themselves to and to control their environment, it is quite impossible to talk and write about physics unless we assume the existence of observers who use the devices—and for that matter who read or listen to our expositions! It is consciousness (using the word not as referring to the 'entity' attacked in Professor Gilbert Ryle's *Concept of Mind* but rather as the general term covering seeing, hearing, thinking, feeling, and so on) that is the indubitable reality. It is 'matter' that is on trial for its life, so to speak.

This last point is important when we try to assess the value of the Thouless-Wiesner fourfold possibilities. One of the possibilities is that the sender's mind may influence directly the body of the receiver; another is that the sender's body may influence directly the mind of the receiver. Now there would be some point in considering these 'possibilities' if the metaphysical doctrine of the body as a 'thing-in-itself' were established, and if the notion of action between a person's 'mind' and his *own* 'body' could be freed from absurdity. But one of the reasons for rejecting the theory that a person is a duality in which a realist or 'thing-in-itself' body is the primal certainty and his consciousness somehow derived or epiphenomenalistic, is the absurdity in which we are landed if we try on this basis to explain the simplest act of perception. What, from this point of view, is supposed to happen when I see a patch of colour? Rays of light, it will be said, are reflected from the patch in straight lines to my eyes, and then microscopic motions, which are different if the patch is red from what they are if it is blue, are propagated along certain nerves to the higher centres of my brain. What happens then? Well, if the motions correspond to red I see red and if they correspond to blue I see blue. But why should I? The redness or blueness has not been conveyed to my brain; all that has been conveyed is motions. The only possible answer, on this set-up, is that I must somehow possess in my mind something analogous to the decyphering tables by which a cyphered message is translated into ordinary language. I translate the motions into the sensuous quality which they symbolise.

Now a person who can believe this is capable of believing anything. It is obviously nonsense. When I see a patch of colour I

see a patch of colour. If I am told that the decyphering is done by my 'subconscious' I must reply that these words convey nothing to me, and in any case are not an explanation. That we are conscious is certain. That there exist material organisms which are things-in-themselves, i.e. which exist out of all relation to any consciousness whatsoever, is a metaphysical hypothesis the truth of which, to put it mildly, is highly speculative. If psychical research is to keep clear of 'speculative associations' it must stick to the 'person to person' account of those phenomena which, as a matter of purely *scientific* method, have in the past been legitimately distinguished as 'telepathic' from 'clairvoyant'.

F. H. CLEOBURY

PRECOGNITION AND PK

SIR,—Dr Thouless's important and otherwise admirably lucid paper 'A Report on an Experiment in Psycho-kinesis with Dice' (*Proceedings*, Part 179) contains one passage which seems to require clarification. I refer to the two paragraphs (pp. 112-13) on the strength of which Thouless makes the following claim for the method he has employed: 'Success in such a PK experiment can only be explained by precognition if we make the *absurd* assumption that when we are doing a PK experiment, we can precognise with an *incomparably* greater accuracy than when we are doing a precognition experiment.' (My italics.) Whether or not this conclusion is warranted, it does not follow from the preceding argument. His previous paragraph leads one to expect that the rival hypothesis which he is concerned to eliminate is that the subject precognises the falls of the machine-thrown dice and causes the corresponding targets to be selected by some means other than PK. In the argument which follows, however, Thouless discusses only the possibility of the subject's precognising the series of events which determined the target-selection. But in what way could this possibility be relevant? What purpose could such precognition have served, since the subject was in any case informed by his senses of the results of the target-selection before he started to try to influence the machine-thrown dice? Surely no one would claim to explain the extra-chance correspondence between the dice-falls and the targets by supposing that the subject precognised *both* of these series of events!

According to the rival hypothesis which ought to be considered, we must suppose that Thouless (*qua* subject) precognised the faces which would preponderate in the ensuing block of falls from his mechanical thrower. The question is whether it was possible for him to influence the selection of the corresponding targets by any

means other than PK. Now Thouless tells us that he selected blocks of targets by choosing one of seventeen 6×6 Latin squares and then permuting its rows and columns several (we are not told how many) times in ways determined by a series of manual dice-throws. Thouless acknowledges that manual dice-throwing is not an adequate test for PK since success may be due to an acquired skill. The rival hypothesis, then, is that Thouless's results were due to precognition plus skill in dice-throwing. We cannot, however, try to apply this hypothesis in detail because we are not told what rules were adopted in permuting the rows and columns of the Latin squares. If, for example, Thouless performed an irregular number of such operations (though I do not imagine he did this) his decisions when to stop might have been influenced by his precognition. But apart from this possibility, skill in hand-throwing might have enabled him to control the arrangement of the targets to an appreciable extent.

To return to Thouless's above-quoted conclusion, surely it is not necessary to suppose that we can only eliminate the PK hypothesis if we assume 'that we can precognise with an *incomparably* greater accuracy than when we are doing a precognition experiment'. In order to explain the results in his machine-thrown series, whose average 'efficiency' was a little below $1\frac{1}{2}$ per cent, we should not, as Thouless implies, need to postulate precognition which was 100 per cent efficient. It would surely be sufficient to postulate an average efficiency of about 12 per cent for *both* the precognition *and* the manual control of the dice used in target-selection.

The best argument against this counter-hypothesis would be to point out that in the earlier experiments for PK when Thouless used hand-throwing his efficiency was less than 1 per cent. (Thouless indicates, however, that it was appreciably higher in the earlier stages.) We can conclude, then, that the results in his machine-thrown experiments must be attributed to PK, *unless* we suppose *either* that Thouless precognises much more accurately when he is not trying to do this, as in a PK experiment, than he does when he is trying, as in a precognition experiment, *or* that Thouless can control the fall of hand-thrown dice much more efficiently when he is not trying to do this than when he is trying. This consideration does make the counter-hypothesis very un-plausible—but not, I think, *absurd*. For as Thouless emphasises later in his paper (p. 117 and pp. 123-4), it seems to be favourable to a person's exercise of psi-faculties that he should not be trying.

C. W. K. MUNDLE

SIR,—Mr Mundle's suggestion as to how PK results with targets determined by Latin squares might be explained as due to a combination of precognition and skilled throwing is ingenious. I do not think, however, that it is a possible explanation of my results.

He asks what rules I adopted in permuting rows and columns of the Latin square. My method was to throw four dice at a time from a shaker. These were marked A, B, C, and D. Suppose the dice fell A4, B2, C2, D1. I put the row starting with 4 as the top row, that starting with 2 as the second row, and that starting with 1 as the third row (ignoring the fall of C because the row starting with 2 was already placed). I would then throw all four dice again. If they now fell A6, B2, C3, D6, I put the row starting with 6 as the fourth row, and that starting with 3 as the fifth row (again ignoring the falls of B and D because these numbers had fallen earlier). The only row now left is that starting with 5, so that would become the sixth row. The columns were then permuted in the same way. This system of permutations was performed once only to obtain each square. There would be no advantage in repeating the process since every Latin square that can be derived from a particular starting square can be obtained by one system of permutations.

Mr Mundle asks an interesting question when he considers what percentage of success in skilled throwing and precognition would be necessary to obtain a given rate of scoring. The figures he gives are, however, far from correct. In saying that a 12 per cent rate of success in each would give a $1\frac{1}{2}$ per cent scoring rate he overlooks two facts. First, to get success it would be necessary to have the row position and the column position both right as well as the precognition, so that a 12 per cent success in each of the operations would lead only to hitting the right block of falls $\cdot 17$ per cent of times (not $1\cdot 4$ per cent). Even this would, however, not mean that the score was $\cdot 17$ per cent. Mr Mundle seems to assume that a hit on the right block would give 100 per cent right on that block. This would be true only if the blocks were of single falls. In my experiment, the blocks were of 12 falls, and I find that the average expectation of scoring by the best scoring target face would be 2 over mean chance expectation. So the expectation of scoring, assuming 12 per cent accuracy in dice control and precognition, would be the negligible amount of $\cdot 03$ per cent (less than one unit in the total score of my experiment IIA).

One could, of course, deal with the question in another way. Assuming that the rows have been permuted, the chance of getting a given column into the best scoring position (assuming 12 per

cent efficiency of precognition and dice control) is 1.4 per cent. But now we are dealing with bigger blocks, and the mean chance expectation of scoring by putting a column into the best scoring position is only about +3 to +4 (on 72 falls), which leads to a lower expectation than that resulting from assuming control of both rows and columns.

If we ask the question the other way round : what expectation of success in precognition and dice control would be necessary to account for a $1\frac{1}{2}$ per cent rate of scoring, we find it is slightly less than 50 per cent. A 50 per cent rate of success in each would lead to a total score of 2 per cent. Did I exaggerate in saying that success by precognition assumed an 'incomparably greater accuracy' in precognition than any we have observed in a precognition experiment?

If a rate of precognition of the order of 50 per cent is far greater than any that has been observed, even this would be of no use on Mr Mundle's hypothesis unless it were accompanied by success in dice control of the same order. We have no evidence of any ability to control dice falls by skill, and if such an ability existed at anything approaching this level of success it could not have failed to be observed. The only way I can see in which manner of throwing might effect dice falls is if one used biased dice and found that one way of throwing maximised the bias effect while another way reduced it to a minimum. The first way of throwing would then give a slightly enhanced expectation of scoring on one of the faces favoured by bias. At best the effect would be small and quite insufficient for the present purpose.

There seems no reason for supposing that by skill one can throw a die so that a given face will be more likely to fall uppermost, but an even more fantastic assumption must be made if one is throwing four dice at a time, that one can control which dice will fall which way. If that can be done to any degree at all, I think we should all agree to accept it as evidence for PK and against skill. I must confess that I did not adopt this method in order to meet the point that Mr Mundle has urged but only to save myself trouble.

The most important question is not whether I have succeeded in making an experimental design which eliminates explanation of PK success by precognition, but whether in principle it is possible to do so. Even if one agreed that Mr Mundle's suggested explanation in terms of precognition and skill were a valid criticism of my experiment, it could very easily be met by eliminating any choice (by dice or otherwise) in the selection of the arrangement of target faces. One could simply take the first Latin square in the Fisher and Yates tables and use that unchanged throughout an experiment. My own method for some time past has been to use a

succession of the same five Latin squares in all experiments, repeating the series unchanged after the fifth. This also was originally adopted as a time saving device but it seems adequate to meet Mr Mundle's objection.

R. H. THOULESS

WATER DIVINING

SIR,—The journal of the Society for Psychical Research for March–April 1951 contains a note by D. P. on the report of what he describes as a ‘carefully designed research project’ which appeared in the Journal of the American Society for Psychical Research for January 1951.

As stated in their report the problem the experimenters set themselves was simply, ‘Can water diviners find water under conditions in which the professional geologist is unable to do so?’

In the light of many years objective experience of dowzers and their craft, I would like to make a few comments on the most obvious fallacies in the rather one-sided contest described in the report.

If the experimenters had known more about the practical results of dowsing they might not have thought it worth while to carry out this experiment, for numerous cases are on record where a dowser has located underground water in an area where a geologist has failed to do so.

In any case the test as arranged was not calculated to solve the problem, as by selecting a site which was practically waterlogged the conditions were such that a geologist would be *certain* of finding water in it.

It is not apparent that any of the twenty-seven dowzers were really possessed of skill and experience and one, ‘an adolescent girl’, could not possibly have been.

The site selected was entirely unsuitable, as water stood a few feet below the surface and no dowser could be expected to work satisfactorily in such an area. This fact alone is enough to damn the whole experiment.

The dowzers were not allowed to work freely. They were blindfolded and manhandled. This sort of personal interference would be enough to disconcert any good dowser. A scientific dowser would get all the information he could from wells in the neighbourhood and in estimating depth and flow would employ a process involving measurements requiring a certain amount of time to carry out.

This test adds one more to a list of many others carried out by uninformed experimenters under unsuitable conditions and from a scientific point of view is misleading and quite without value.

(Colonel) A. H. BELL

President, British Society of Dowzers

SIR,—Your reviewer speaks very highly of the recent American field experiment in dowsing. He concludes that 'if a test of this kind is not a valid test of dowsing then we may as well throw up the sponge and declare that the phenomenon, as claimed, is untestable'. I wish I could agree with your reviewer's estimate of the experiment and with his conclusion, but I cannot. The names of the sponsors of the experiment are a guarantee of a conscientious and sincere piece of work: but that, alas, is not enough. It is surely a rather elementary principle of scientific methodology that when the attempt is made experimentally to reproduce an alleged phenomenon, the conditions of the experiment must be the conditions under which the phenomenon is alleged to occur. There can be an exception to this rule only if these conditions are such as to make verification impossible. Thus, in our field, if a medium claims that certain phenomena will occur in his presence only in the dark and in the absence of control, the investigator is obliged to suspend judgment, and is perhaps even entitled to reject the claim on the ground that it is untestable. Dowsing fortunately is not an alleged phenomenon of this order. Nothing is easier than to put dowsing practically to the test under natural conditions: nothing is easier, but unfortunately nothing in psychical research is more expensive. That is the crux of the matter, that is why after many years of effort I have never been able to organize a proper investigation. The American group have tried hard to do it cheaply, but I fear that they have produced a quite inconclusive report.

THEODORE BESTERMAN

SIR,—The twenty-seven dowzers accepted the conditions and 'in spite of . . . minor difficulties . . . it was apparently the belief of all that the rod responded normally to underground water'. The American team made ultra-modest claims for their experiments, as I was careful to emphasize by extensive quotation.

In *The Divining Rod* by Sir William Barrett and Theodore Besterman (1926) a whole chapter is devoted to experiments with dowzers who were blindfolded and often 'manhandled'. These experiments were for the most part regarded as successful. It is difficult to resist the conclusion that it is only the *unsuccessful* dowsing experiment which evokes criticism.

Dowisers and their sponsors could help us by setting down an irrevocable statement of the conditions they will accept for a practicable definitive test. The test should be designed to eliminate or allow for geological knowledge or inference on the part of the dowser.

DENYS PARSONS

THE STATISTICAL EVALUATION OF GROUP EXPERIMENTS

SIR,—Dr Schmeidler is perfectly right, and except on the academic issue I was quite wrong. Although it is true that a correct statistical method must always treat the targets as variable and the guesses as given, I was wrong in concluding that 'we cannot expect to get more significant results by using more percipients to guess at the same targets'. We *can* expect to, because a group opinion (guess) will, in general, be better than an individual opinion—random effects cancel out while systematic effects accumulate. The most that can be said is that, however many percipients we use, we cannot on any legitimate statistical method get any result better than would be obtained by a single percipient scoring at the level of 100 per cent success (except conceivably by bringing in further external evidence) and that up to this limit the introduction of more percipients gives diminishing returns. However, this peak is so high and for normal material the returns diminish so slowly that these limitations have almost no practical importance. I therefore accept all that Dr Schmeidler says, apologise for raising a red herring, and thank her for not trouncing me as severely as she would have been quite entitled to do.

CHRISTOPHER SCOTT